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ABSTRACT

This paper describes the events following the news of Oersted's discovery of the effects of a current of electricity on a magnetic needle. It focuses on Biot and Savart's law which expresses the force between a current element and a "molecule of magnetism" that can be deduced from Ampere's law of interaction between two current elements. Experiments and equipment for performing the experiments are described and illustrated. (SA)

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The Law of Biot-Savart

(Experiment # 18)
The Beginnings of Electrodynamics
(Additional Notes)

History of Physics Laboratory
Barnard College

SE 020 870

THE LAW OF BIOT-SAVART

The Beginnings of Electrodynamics

Additional Notes
(c.f. Blue Book pp. 88-98)

Experiment No. 18

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Samuel Devons
July 1975
Barnard-Columbia History
of Physics Laboratory

THE LAW OF BIOT-SAVART

The Beginnings of Electrodynamics

Additional Notes
(c.f. Blue Book pp. 88-98)

I. Historical

J. B. Biot lost no time in addressing himself to the new phenomena. Less than three months after Arago had brought the news of Oersted's discovery to the Academie at Paris, Biot, with the assistance of Felix Savart, is reporting to that same body the results of measurements from which he is able to

> "...express, rigorously, the action experienced by a molecule of magnetism - Austral or Boréal -, placed at any distance whatever from a very fine cylindrical wire of 'infinite' length which has been rendered magnetic by the Voltaic current."

> "By means of this law of force", Biot continues, "one can predict and calculate quantitatively ('en nombres') all the motions imposed by the 'conjuctive wire' [Oersted's terminology] on any magnetic needle, whatever the direction of the wire with respect to the needle."(1)

This rather grand claim was based on the simple result that the force at a point in the vicinity of an 'infinite' straight wire was inversely as the distance of the point from the wire. Biot's claim did not go unchallenged: the adequacy of his experimental procedures was criticised; the sufficiency of the evidence as a basis of a general law was questioned; there were even doubts as to the consistency of Biot's laws with the well-established (Newtonian) principles of mechanics. Ampère was the main critic.

The method of timing the oscillations of a (small) magnetic-needle to measure the magnetic force (field) was in 1820, an old established one - going back to the early 18th century. It had been extensively used, e.g. by Biot himself, and by Von-Humboldt in exploring the earth's field, and by Biot and Gay-Lussac in their adventurous ascent by ballon (to a height of 13,000 feet in 1804) to examine the change of the earth's magnetism with altitude. In the context of terrestrial magnetism this method had achieved a high degree of precision and reliability. But its application to experiments with Voltaic-circuits presented difficulties of a new sort. The earth's magnetism could be assumed more-or-less constant over time intervals long enough (minutes or even hours) to complete

measurements; by contrast the source of the galvanic current - the Voltaic pile - could change very significantly and even erratically during the course of any experiment. Again the earth's magnetism would be considered uniform over distances far greater than the size of the instrument (the magnetic-needle) used to explore it: this was not likely to be true, as Oersted already recognized, for the magnetic force near a wire. Experience with traditional (terrestrial) magnetic measurements was then no guarantee of success in these new circumstances: and indeed Biot and Savart's early hastily undertaken measurements - even if correct - were insufficient to inspire full confidence.

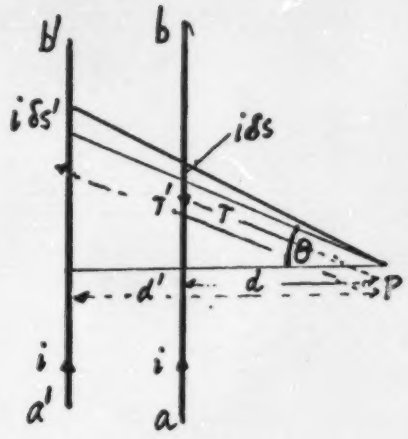
Ampère, from motives not unmixed with personal rivalry, argued that the method used by Biot and Savart was in principle incapable of precision; or at least that his own methods - which did not depend on the constancy of the source of current - were bound to be superior. Ampère's basic method was to balance against each other two electromagnetic (or electrodynamic) forces, both produced by the self-same current, so as to produce a null effect. Biot, by contrast, measured the forces, mechanically, and successively in different electromagnetic circumstances, and he attempted to compensate for unsteadiness of his Voltaic apparatus by appropriate interpolation of the measurements ("une méthode d'alternatives").

(It is of interest to note that subsequent, far more refined electrodynamic measurements - for example those of Wilhelm Weber in the period 1830-1860, were a sort of hybrid between the superior "principe" of Ampère and the more realistic "practique" of Biot. Electrodynamic forces are measured at the same time as the currents are "measured" by some standard instrument - a galvanometer of some sort. In effect one is then comparing two electrodynamic forces due to the same current. But the flexibility of this arrangement - the two forces do not have to literally balance each other, as in Ampère's experiment, yet the comparison of successive measurements, such as Biot's, is avoided - is clearly evident in the far greater precision attained, as Weber himself well realized. Of course this development of techniques could only occur after the general nature of electrodynamic forces had been discovered and explored.)

Whatever may have been their experimental reliability, there was no question about inadequacy of Biot's measurements with an "infinite" straight wire as a basis for a general theory. LaPlace's argument clearly demonstrates this. A "theory" capable of predicting the force due to any wire must - or so it seemed - be based on a law which expresses the contribution from each infinitesimal element.

Assume the law-of-force of the form:

$$\delta F_p \propto \frac{i.\delta s}{r^n} f(\theta)$$



r is the distance between P and the element of the wire δs. n is an unknown exponent: $f(\theta)$ an unknown (dimensionless) function of angle (between r and δs: only <u>one</u> angle is significant.).

Compare the force due to the two wires (F, F') by consideration of corresponding elements, as shown. Then

$$\delta F_p' : \delta F_p = \frac{i\,\delta s'}{r'^n} f(\theta) : \frac{i\,\delta s}{r^n} f(\theta)$$

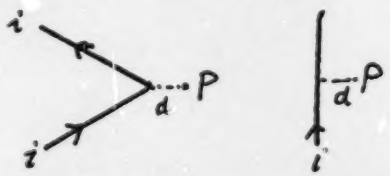
(since θ is the same for both elements, and $\delta s'/\delta s = r'/r$).

The same ratio must apply to the integrated effect of the whole wire; i.e.

$$F_p'/F_p = (d/d')^{n-1}$$

Biot and Savart's experimental result - force inversely as distance - demonstrates then only that $n = 2$, but gives no information about the angular dependence $f(\theta)$: clearly an insufficient basis for a comprehensive theory!

To meet this theoretical refutation of their claims, Biot and Savart turned to measurements of the magnetic force produced by a pair of "semi-infinite" wires meeting at a common apex. These later measurements took the form of a <u>comparison</u> of the force due to such a wire and the infinite straight one, at equal distances, and for equal currents in the two. This of course can only be some function of the angle ϕ. (There is no scale of distances in such arrangements). For small ϕ, clearly the force approaches zero: for $\phi = \pi$ the two forces are equal.



In the first report of measurements with 90° angled-wires in 1821[2], Biot states the result is consistent with a force proportional to the angle between the branches of the wire; and that this result is, moreover, consistent with $f(\theta) = \sin\theta$, in the basic law-of-force of an element. The inconsistency of these statements was pointed out later, in 1823, by Savery who gave the correct dependence on the angle Φ, namely, $2 \tan. \Phi/4$.[3]

In the third edition of Biot's Traite d' Physique (1823), where the first full account of the experiments is presented, he gives the result of a comparison of a straight and a 90° angled wire, and using magnetic needles of lengths 20 and 10 mm. He reports for the ratio of the forces two sets of values, with means of 0.806309 (with 10 mm. needle), and 0.840780 (with 20 mm. needle). Averaging this pair of values, he obtains 0.823694 (!), which is to be compared with $2 \tan 22.5° = 0.828427$.

He is apparently not fully satisfied with this degree of concordance of theory and experiment: the discrepancy (!) he attributes to a slight departure from the ideal geometrical configuration of the angled wire - leading to a correction of the order 1/200. His final comparison is Experiment: 0.827545 (the mean of 0.845451 and 0.809641 !) which "coincides" — "presque rigoureusement" - with the theoretical 0.828427! (Biot's original "theory" would have predicted, for this ratio, $\pi/4 = 0.7854$.) How this "rigorous" agreement of experiment and theory impressed Biot's contemporaries is hard to say. In any event Ampère - who never quoted numerical results from his experiments; in effect there weren't any - has his own grounds for suspicion. Biot's definitive results and his "fundamental law": $F \propto i\delta s \sin\theta / r^2$ only appeared after Ampère's extensive exposition of his own electrodynamic theory. All the earlier reports of Biot and Savart had been marred by incompleteness, faulty theoretical reasoning, or incorrect conclusions based on imprecise measurements! Added to this, Ampère and Biot took diametrically opposite basic "philosophical" viewpoints regarding the nature of the electromagnetic force.

On one issue, the dependence of the interaction on the current, Biot and Ampère seemed to agree. Both assumed the magnetic force proportional to the current, and for both it was a tacit assumption, rather than one based on experimental evidence. How might such an assumption have been tested? Could current have been "measured" independently of the magnetic force, so that the proportionality of the latter be established? In principle, perhaps; electrochemical decomposition might have been used as such an independent current "meter"; but in 1820 it was far from clear that the quantity

of chemical decomposition depended only on the quantity of electric charge and no other physical circumstances. (Faraday announced his Laws of Electrochemistry in 1833). Qualitatively, the heating of a wire provided a rough indication of the magnitude of the current flowing, but in no sense a quantitative measure. An alternative, though limited, procedure might have been to examine the separate and combined magnetic effects of two currents flowing, independently, in two adjacent wires. It would have been a reasonable assumption - and one consistent with observation, - that the proximity of the two currents did not affect the magnitude of each separately. In fact Biot and Savart did make some, inconclusive, attempts to compare the effects produced by bundles of wires, with those of a single wire, and a hollow cylindrical conductor. But since the different conductor combinations were used with the same Voltaic battery, and the relationship between current flowing and the properties (resistance) of the conductors used was wholly unrecognized (Ohm published his "law" in 1826/27; but hardly then in a form sufficient to provide a clear basis for experimental practice), these measurements provided a negligible experimental basis for the assumption that the magnetic force was proportional to current.

In essence, the assumption of proportionality was then virtually a definition of current. That such a definition could provide the basis of a self-consistent theory was implied tacitly. Conflict between this definition of current, and any other notion of current was unlikely precisely because all other notions of current were, quantitatively, so vague. Nevertheless the assumption was of somewhat different significance for Ampère and Biot. In Ampère's electrodynamics - where the current-element is the basic, in a sense irreducible, element, definition of current via its electrodynamic forces is quite logical - although verification of the strict proportionality of current-current and current-magnet interactions would still be required. But from Biot's standpoint - where the magnetic effects were due to some "modification imprinted on the particles of the metal by the electric current" - it was quite an arbitrary physical assumption, or a surprising conclusion of experiment - that the same current, in different metals "imprinted" exactly the same modification on the "magnetic" molecules.

Neither Biot nor Ampère seem to have gone much further than Oersted in examining the dependence of the magnetic force on the material of the conductor. It would seem to have been a straightforward matter to have examined the magnetic force produced by conductors of two different materials, connected in series in the same circuit. But whether because of the tacit assumption that the force must be proportional to the current, or because of the relatively primitive and unquantitative practice in the use of Voltaic

circuits, no such measurements seem to have been made. A decade or so later, in examining the electromotive forces produced by varying currents, Faraday makes just such a test.

Biographical Note

Electricity was not the central interest of either Biot or Savart. Biot's energy and catholicity took him into many fields: an extensive experience in terrestrial magnetism and optics, where his most important contribution was, perhaps, the discovery of optical rotatory activity.

FÉLIX SAVART'S (1791-1841) only excursion into electric or magnetic research appears to have been as junior partner in the Biot-Savart collaboration. His sustained interest was in accoustics; he wrote memoirs on such topics as the voice of birds and the modes of vibration of solids. F. Savart should not be confused with his contemporary FÉLIX SAVARY (1797-1841) who was more a mathematical than an experimental physicist. As a protége of Ampère he contributed to the working out of the detailed mathematical implications of Ampère's electrodynamical principles.

In particular in 1823 Savery showed that the law proposed (in 1820) by Biot and Savart ($\delta F \propto i\,\delta S \sin\theta / r^2$) expressing the force between a current-element and a "molecule of magnetism" (which was equated by Savery to the extremity of an Ampérian Solenoid) could be deduced from Ampère's law of interaction between two current elements. At the same time he pointed out the contradiction between the law proposed by Biot and Savart and what, at the time, they reported as the results of their experiments with the angled wire (c.f. p. 3).

II. Biot and Savart's Experiments

There are as already indicated several successive accounts of Biot and Savart's experiments - each with a somewhat different conclusion and of somewhat different historical significance. The fullest account, that of 1823, illustrates both the potentialities and actualities of this type of measurement at the time. Assuming the source of current was similar to that used in earlier experiments, it comprised a battery of ten copper-zinc Voltaic cells, each with a plate area of some 100 cm^2, arranged, presumably, in series. It appears from the results of their measurements that the current was about 1 amp (at least), which represents quite an efficient battery construction.

The magnetic force is measured by timing the oscillations of a small, magnetized rectangular parallelipiped, two sizes of which were used: 20 X 10 X 1 mm. and 10 X 10 X 1 mm. The reasons for using a "needle" of such a shape - although discussed - are not clear; it is recognized that the effective length - i.e. the distance between the effective poles - of the "needle" should be small in comparison with the distances between needle and wire. The latter distances range from 15 to 120 mm.: the effective distance from center to pole, for the smaller of the two needles was estimated at about 3 mm.

Measurements of the variation of the magnetic force with distance from the wire are made with the magnetic force of the Earth "exactly" balanced out by the auxiliary compensating magnet - that is how the procedure is described. Yet "small" discrepancies, especially at large distances ($\gtrsim$ 100 mm.) where the force of the wire is weak, are attributed to imperfect balance. There should, of course, have been no difficulty in correcting for this effect experimentally, (using the period of oscillations without current in the wire). However the style is not unfamiliar - going back to Coulomb at least: to assume that discrepancies between observation and the "exact law" can be logically accounted for, rather than empirically removed.

In another series of measurements, which as Biot presents them[4] are ostensibly to test the theory that the wire acts separately on "free elements of austral or boreal magnetism" (this is the language used to describe the magnetic properties of the needle, etc.), oscillations are timed with the magnetic force from the wire alternately in the same and in opposite directions as that of the Earth. Denoting these times by T_+ and T_- respectively,

and by H_E and H_I, the forces due to Earth and wire, then clearly (assuming $H_E > H_I$):

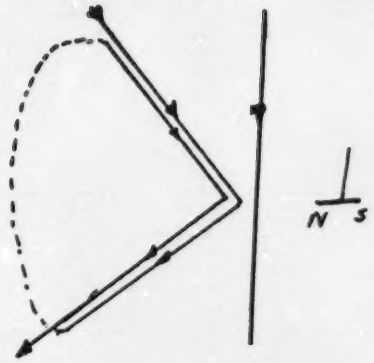
$$\frac{H_E + H_I}{H_E - H_I} = \left(\frac{T_-}{T_+}\right)^2 \qquad \text{or} \qquad \frac{H_I}{H_E} = \frac{(T_-/T_+)^2 - 1}{(T_-/T_+)^2 + 1}$$

Thus a measure of the Oersted force in terms of the Earth's is obtained for a particular Voltaic arrangement (but otherwise unspecified current). (From such measurements one can deduce that Biot and Savart used currents of $\sim$ 1 amp.)

In a somewhat confused report Biot[5] attempts to compare the forces due to a current in a hollow tube with those in a solid wire. The two are mounted close together and the current passed alternately through the tube, the wire or both connected in parallel. (A superior method would have been to pass the same current up one and down the other; and so measure the difference!). It was recognized that the tube with greater conductivity carried most of the current (when connected in parallel with the wire); and could also carry greater current than the wire, when each is used separately - as the results seemed to indicate. A 1/r dependence (r is now the distance from the axis) was found for the tube as for the wire. Biot's general picture of the Oersted effect is clearly revealed in his interpretation (of the greater force from the tube) that it

> "may be due either to a greater circulation of electricity, or to a more favorable molecular magnetization which results from it."(6)

Recognizing that measurements with a straight wire cannot yield information about the angular dependence in the "fundamental law" (c.f. p. 3), and that his earlier experiments were none too satisfactory, Biot finally addresses himself to the comparison of an angled and a straight wire, "avec une extreme rigueur". He uses an angle, symmetrically disposed about the horizontal of 90°, for which the force is expected to be (only) 0.414214 (sic) (i.e. tan=22° 30') of that of the straight wire. To increase the effect he folds back the wire on itself so as to double the force



(apparently as indicated). (There is no explicit mention of a check that such doubled wires - straight or angled - yield twice the force of the single wire.) This angled wire is mounted so that its "vertex" is very close to the straight (vertical) wire and the plane of the whole arrangement arranged perpendicular to the meridian. Measurements are made by passing the current alternately through the straight and angled wires (and in both directions). The wires are carefully specified to be of the same material and diameter ("perfectly identical...from one and same continuously drawn piece"). Yet again this is no assurance that the currents in the two wires, of possibly different lengths and therefore resistances, are the same; (presumably, due to predominance of the internal resistance of the battery, the differences were small.) In any event, after small corrections for the slightly imperfect geometry, theory and experiment coincide "presque rigouresment."

Throughout all these investigations, as Biot repeatedly reminds us, it is essential to reduce the influence of battery fluctuations by appropriate interpolation of measurements - by employing what he calls, "la méthode des alternatives." Biot also remarks (1823), that he "only discovered successively the absolute necessity of all the precautions" that he describes in his experimental account, and that later experience often indicated a better way of doing an earlier reported experiment. It might, for example, have been easier to move the current-carrying wire and leave the oscillating magnet, its support and the compensating magnet fixed - rather than vice-versa. Be that as it may, in the laboratory experiments outlined in the next section, it is the original Biot-savart measurements that are reproduced.

III. Laboratory Experiments

Two sets of measurements are indicated;

i) the verification of the "1/r" law

ii) a comparison of the magnetic effects of the current-carrying wire and the Earth.

Extensions to other investigations, for example the study of the angled wire, are straightforward.

To appreciate the significance of the experiments in their historical context, as well as to gain the physical feeling for what is needed to generate appropriate currents (of the order of one ampere), the experiment should be made with Voltaic cells. The actual measurements are of course greatly simplified if a given, constant source of current is used. Circuit and battery arrangements are similar to those described for Oersted's experiments(7).

The oscillating magnetic "needle" can be either of the rectangular form (say 0.5 X 0.5 X 0.1 cm) or a magnetized piece of steele needle $\sim$ 0.5 cm. long. The suspension is the usual single nylon thread.

Preliminary Procedure

i) Check that supporting nylon-thread has negligible torque. This can be done by timing the oscillations (if any!) using a piece of brass (non-magnet!) similar in shape and size to the magnetic needle which will be used.

ii) Without any currents, check the isochronism of the oscillations for various amplitudes: e.g. 10°, 25°, 50°. (Only small oscillations are simple-harmonic. How small is small?)

iii) Since the magnetic field of the wire is not uniform there will be a force as well as a couple on the needle. Check that this does not seriously interfere with measurement, e.g., of the position. With the needle as close as possible to the wire, observe whether its distance from the wire changes when the current is switched off and on. You may wish to do this with different orientations of the needle-wire with respect to the meridian.

"1/r" Law

i) Establish the magnetic meridian line (Make sure no magnetic materials are nearby!). Arrange the apparatus so that the line joining the auxiliary magnet to the position of the oscillating needle is parallel with the meridian. Clamp in this position.

ii) Observe (without detailed measurements) how the oscillations of the needle change as the auxiliary magnet is brought up close. Note its position where the oscillations appear slowest. Time the oscillations for a sequence of positions of the auxiliary magnet. By interpolation, identify the position where the two forces, that of the Earth and the magnet, cancel.

Time the oscillations (if any!) in this position. Check that there is no significant change when the carriage (carrying both needle and auxiliary magnet) is moved to another position.

iii) Bring the (vertical) current-wire close up to the apparatus and ensure that it lies along the line-of-motion of the needle generated by movement of the slide.

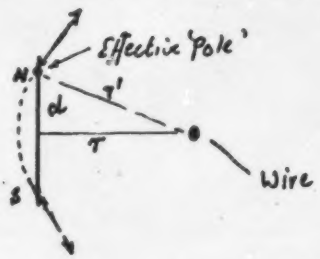
iv) Close the circuit. Observe the position of equilibrium of the needle. Time the oscillations for some reference distance (say 3 cm.) between needle and wire. Repeat this measurement several times; opening and closing the current-circuit between each measurement. (Reproducibly good contacts essential!)

v) Make similar measurements at various distances, say 1 to 10 cm., interpolating a measurement at the reference distance between each successive measurement. (Biot's "method of alternatives")

vi) If the periods of oscillation are not <u>much</u> smaller than the longest period in ii) above, some correction for the residual field may be necessary. To make this it will be necessary to know whether the current-produced field is parallel or antiparallel to the residual field. This can be ascertained by reversing the direction of current flow and retiming the oscillations. This check should be made at a fairly large distance from the wire. (Why?)

vii) In analysing the results, the period corresponding to any particular distance can be compared, with the average of the measurements at the fixed reference position, made just before and after the measurement in question. Since the period T is related to the "force" H, by $H \propto 1/T^2$ (subject to correction, vi) above), the relationship between T and r can be displayed directly. ($T^2 \propto r \rightarrow$ implies $H \propto 1/r$)

(Note: Since the dimensions of the needle are not infinitesimal in comparison with r, at small r, one should not expect $H \propto 1/r$ to hold at close distances: (a) the effective "pole" forces are not co-linear, and (b) the distance from the wire to the poles (r') is greater than to the center of the needle (r). The "effective" field is less than the field at r by a factor of $\sim (1 - 2d^2/r^2)$, where 2d is the separation of the poles (something less than the physical length). For $d \sim 0.3$ cm., r=1cm., the "correction" is quite appreciable ($\sim 20\%$), and easily seen in careful measurements.



Comparison of Magnetic Force of Earth and Current

i) The apparatus is carefully aligned so that the line from the current-wire to the oscillating needle is perpendicular to the meridian line. A separation of, say, 3 cm. is convenient. The auxiliary magnet is removed.

ii) Oscillations are timed a) with no current, b) with current flowing, c) with no current, d) with reversed current flowing and e) with no current.

iii) The measurement can be repeated at different positions.

Note: Reproducibility of current-circuit conditions should have already been checked ((iv) above).

iv) Some checks might be made that force measured is independent of the particular manner in which the connections between the vertical wire and battery are made; provided that...?

The above directions refer implicitly to the apparatus as shown on pp.15/18. As already mentioned, equivalent, and mechanically

perhaps simpler, arrangement would be to keep the needle-suspension and auxiliary magnet fixed and move the wire as necessary. Apart from questions of historical veracity, the arrangement as shown does have the useful feature of directly demonstrating that the compensation between auxiliary magnet and Earth's field is independent of position in the uniform field of the latter.

IV. Apparatus

The main piece of apparatus is depicted on pp. 15/18. It comprises an arrangement whereby the suspended needle together with the auxiliary magnet can be moved smoothly and by a measured distance, by means of a simple slide-pulley-string combination. The needle is protected from draughts by a transparent (lucite) cover. The whole is mounted on a turn-table which can be clamped in position when set at the proper orientation.

The current-carrying wire (only a straight wire is shown) is mounted on a wooden support-stand, whose base is fitted with levelling screws (used with an attached plumb-line), and weighted with lead underneath for stability.

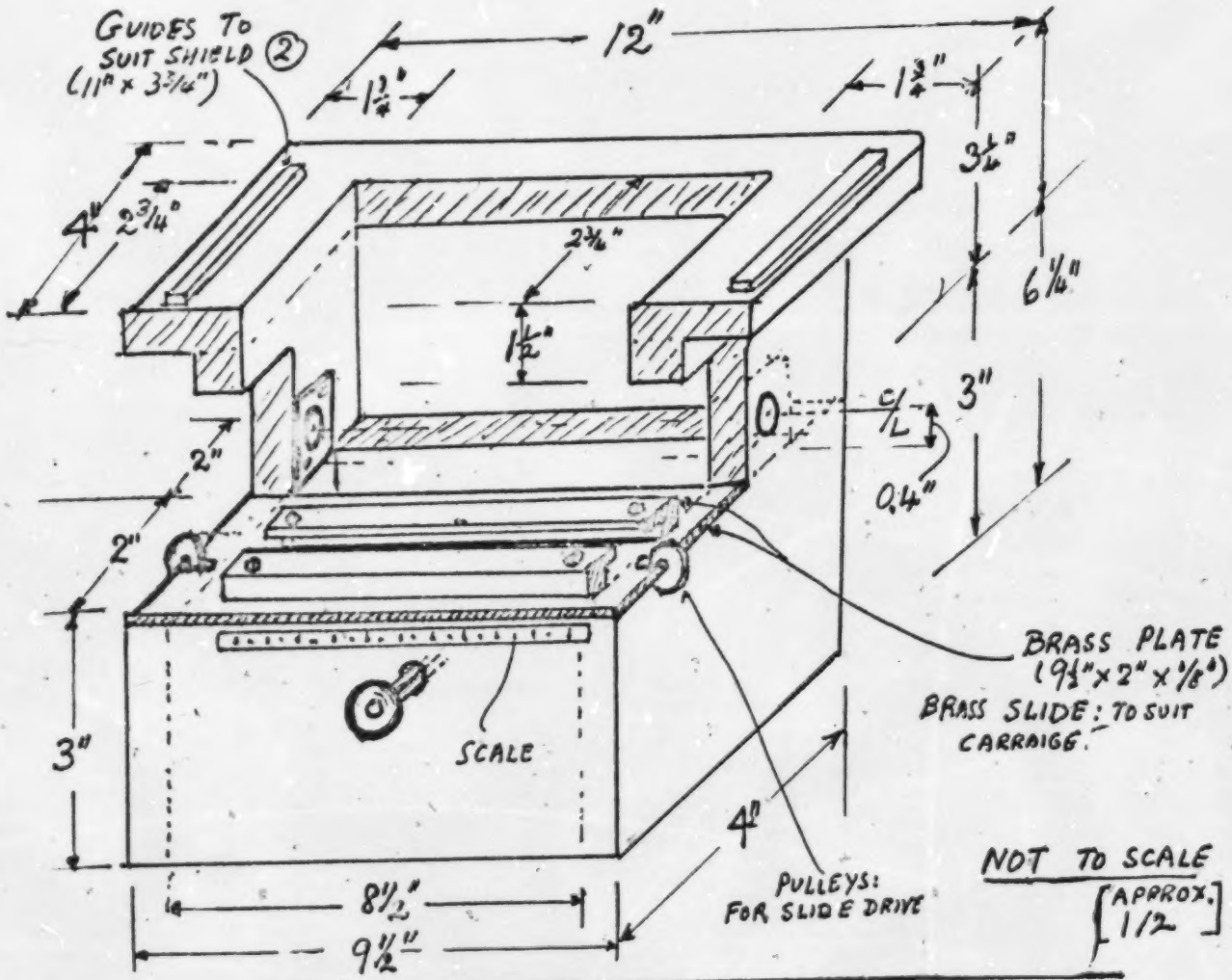
A small Alnico magnet 1" X 1/8" X 1/8" provides a suitable compensating magnet. (Neutral point at about 5")

Electrical-circuit details are described in the Barnard-Columbia History-of-Physics Laboratory notes for the Oersted experiment. (No. 16, July 1975)

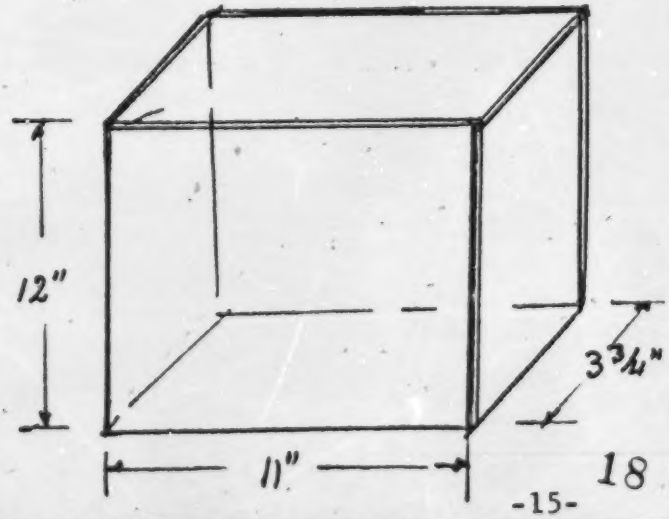
BIOT-SAVART

FRAME ①

ALL MATERIALS NON-MAGNETIC: HARDWOOD FRAME; BRASS/AWM: WHERE SPECIFIED.



COVER-SHEILD ⅛" LUCITE ②



(INSIDE DIMENSIONS)

(¼ APPROX)

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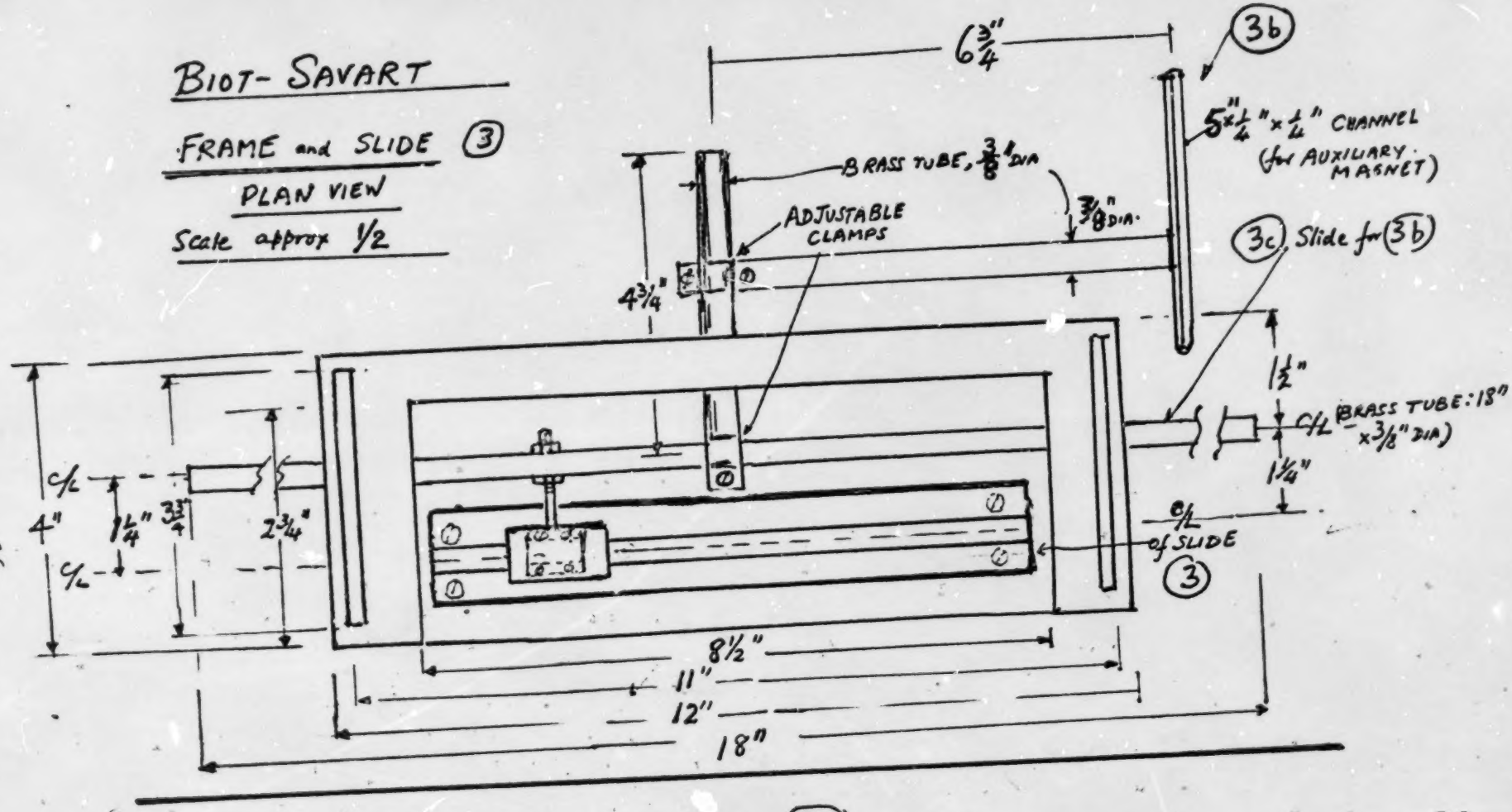
BIOT-SAVART

FRAME and SLIDE (3)

PLAN VIEW

Scale approx 1/2



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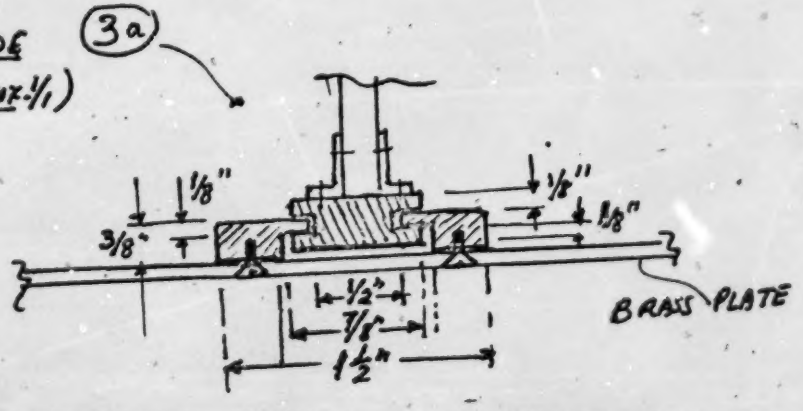
DETAIL of SLIDE AND RAIL (APPROX. 1/1)



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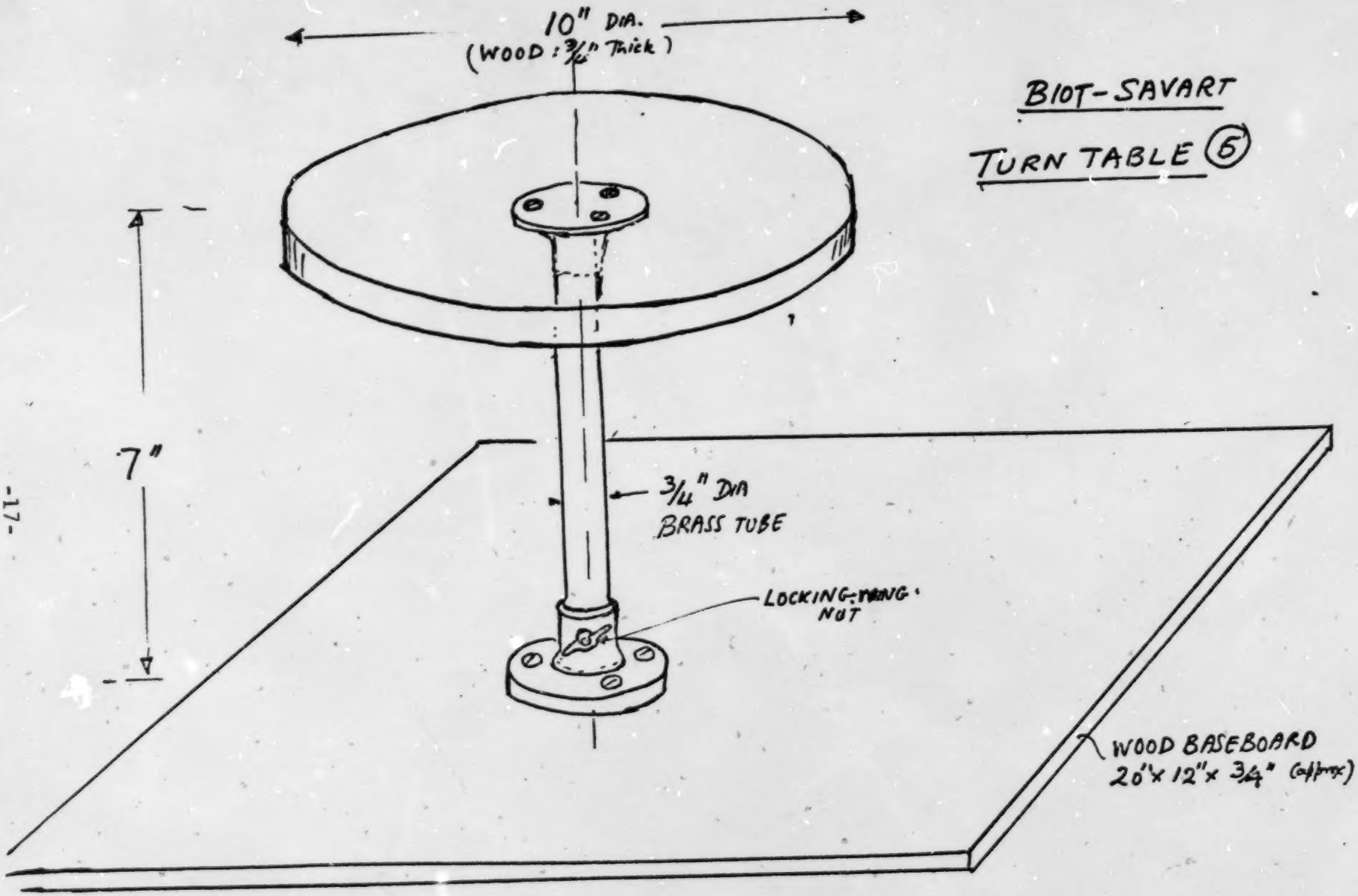


BIOT-SAVART

TURN TABLE (6)

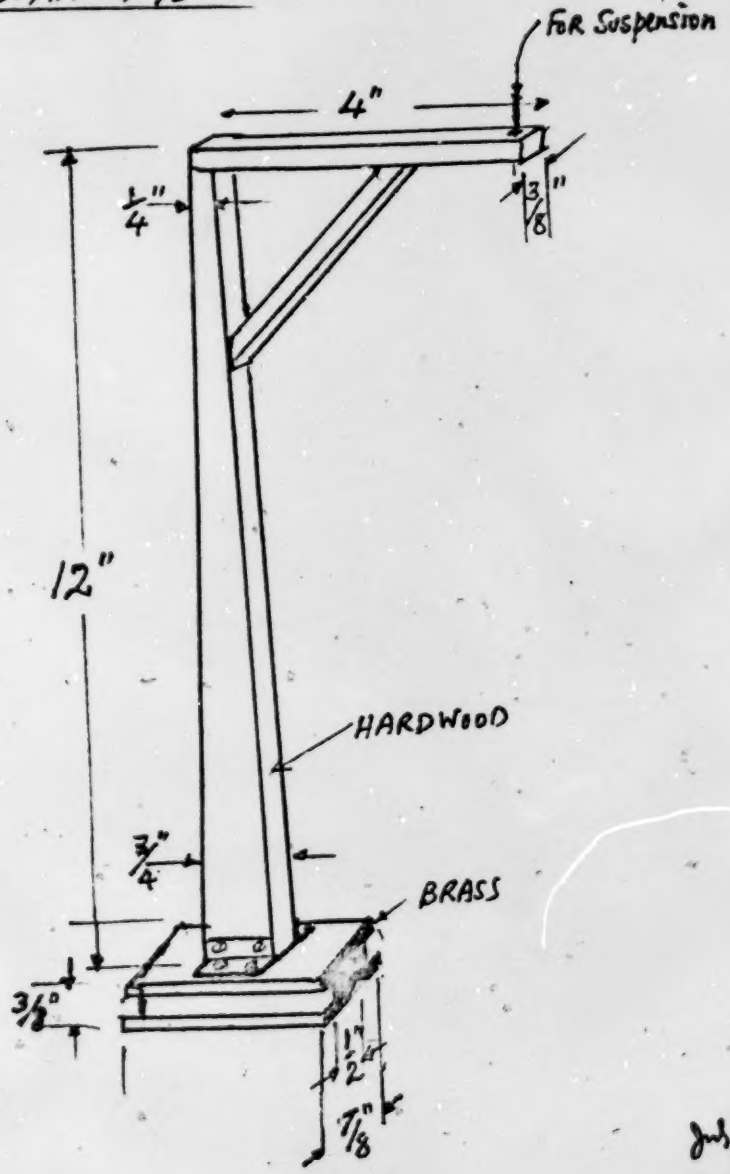
-17-

JLG 11/15

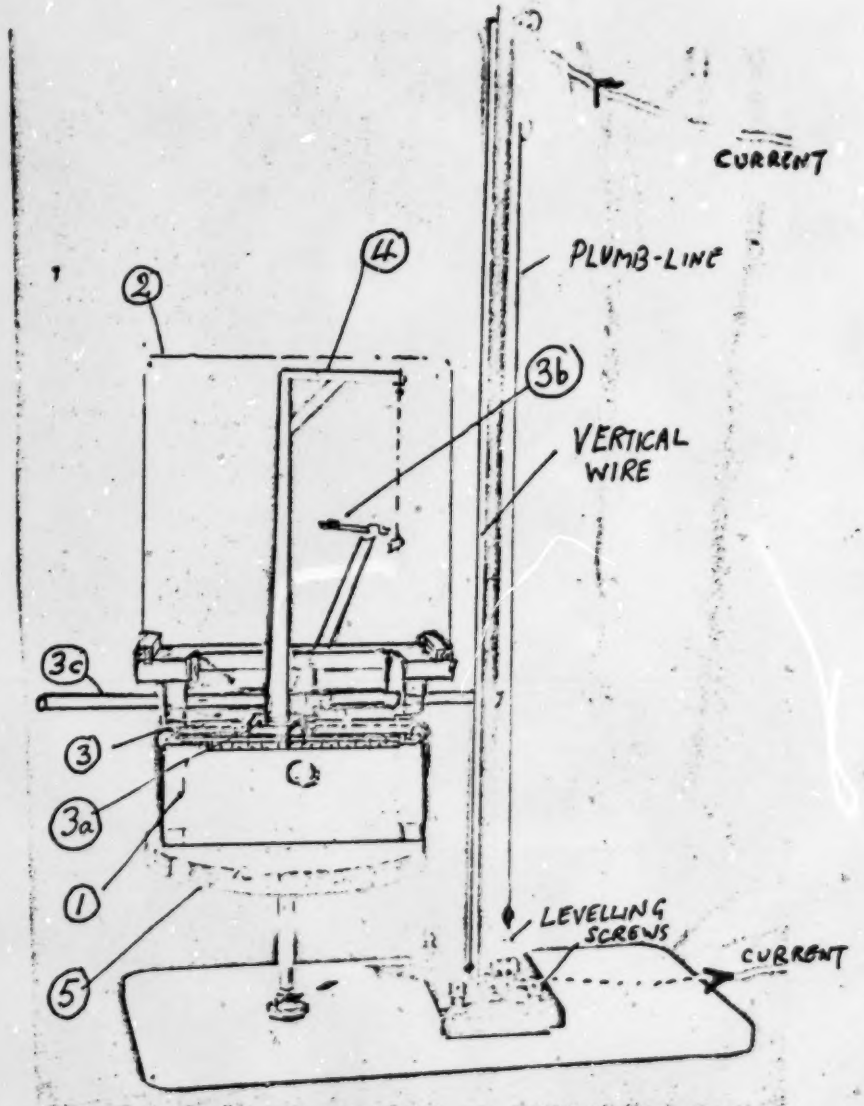
BIOT-SAVART

SUPPORT POST (4)

SCALE: APPROX ½



ASSEMBLED EQUIPMENT.



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(3) F. Savery. Academie des Sciences. Feb 1823. Reproduced in Mémoires sur l'Electrodynamique, Vol. II, pp. 338-375.

(4) Biot. Ref. III. pp. 103-108.

(5) Ibid. pp. 108-112.

(6) Ibid. p. 113.

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